U.S. AIR FORCE
Project RAND
RESEARCH MEMORANDUM

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PROJECT RAND

RESEARCH MEMORANDUM

TEN COMMON PITFALLS

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PREFACE

This is the preliminary dreft of a report which is being circulated for information and comment. We hope eventually to incorporate it into a book and would, therefore, appreciate any comments, criticism, ideas, and examples that readers may have. This draft began as a verbatim transcript of an informal talk and, despite some rewriting, it probably still suffers (like many such talks) from being "fashionable." We are aware that it has a number of other weaknesses and assume there are still others of which we are not aware. We also hope to give it a thoughtful and leisurely review but are deferring this until we get some outside criticism.

In order to give the reader a feeling for the place this material might have in the book a table of contents of the book is given on the next page. A more complete introduction and list of acknowledgments, etc. are given in RM-1829.

H. K.
I. M.

MILITARY PLANNING IN AN UNCERTAIN WORLD

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INTRODUCTION

Probably no applied professional group is so intensely and continuously concerned with methodological and philosophical questions as Operations Analysts and Systems Analysts. Partly this occurs because it is important to be clear on methodological points and partly it is undoubtedly just the normal introspection to be expected in any new field. However it is hard to avoid the feeling that much of this self-questioning is caused by a sort of mass inferiority complex or at least a general sense of insecurity.

Assuming that this insecurity exists we would conjecture that it is due to at least two causes:

1. The somewhat nebulous and unspecialized nature of most of the work makes it hard for practitioners to obtain the automatic deference and acknowledgment that the more esoteric professions get as a matter of course. This sometimes causes the Systems Analyst to try to appear more technical or specialized than necessary. There will be some comments about this in the Miscellaneous Comments chapter when we discuss Systems Analysis as a profession.
2. A correct (if sometimes subconscious) recognition that an extraordinarily high percent of the work done in this field is somehow not quite passable. We believe that this last is partly due to the intrinsic difficulty of doing a good job and partly because of the prevalence of certain common mistakes.

This chapter is concerned with identifying and discussing some of these common mistakes. It is therefore a methodological chapter but unlike

the section we have just finished it is philosophical rather than technical. Because it is not concerned with either tools or substantive questions some readers may find it tautological or superficial. We feel however that it is useful to provide the reader, whether he is a consumer or a practitioner, with a sort of checklist of common pitfalls. Hopefully it will at least alert him to the things to look for in an analysis.

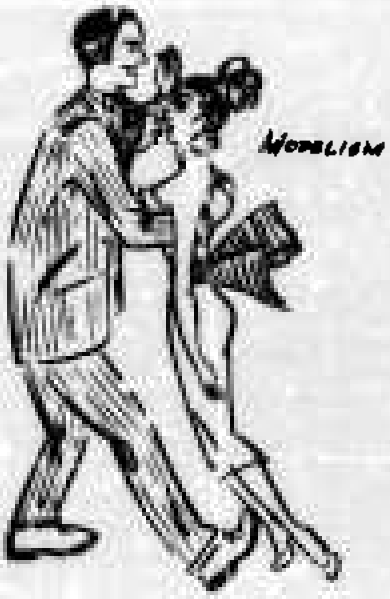
This chapter is often deliberately doctrinaire. It should be clear to the reader that any doctrinaire rule which attempts to provide a guide to -- analysis without looking at the specific problems will be misleading a certain percent of the time. The best that can be hoped for from a doctrinaire rule is that it will do more good than harm. Insofar as the reader is willing to trust our authority he should at least be worried when he sees an analysis which violates these admonitions.

Many of the points that we make may seem obvious to the point of banality. This they may be; but obvious or not it turns out that many of our colleagues disagree with us; a few even think some of our comments as scurrilous, if not libelous.

The material of this chapter overlaps somewhat with Part One. In fact, the first four chapters of Part One and the first four pitfalls are practically in one-to-one correspondence. Chapter 1 of the example is chiefly concerned with Models (and Modelism), Chapter 2 with Statistical Uncertainty, Chapter 3 with Real Uncertainty, and Chapter 4 with Enemy Reaction. The reader might wish therefore to skim relevant parts of the first part if he would like specific examples of what we are discussing.

CONTENTS

I. MODELISM



We shall start by considering what is to many people, the heart and soul of Systems Analysis--the use and abuse of models. We have already explained that it is necessary to use idealized models which abstract essentials and make assumptions explicit. It is, however, a frequent pitfall to abuse this modelizing by being more interested in the model than in the real world.

Instead of designing the analysis so that it really can answer some important policy questions (which may get one into some mathematically untidy questions) many analysts prefer to study only the interesting (to them) portions of the whole problem. They often end up by studying an irrelevant or overidealized question. Or what is sometimes almost as bad, the question that is being studied is relevant but not complete. This last could be useful if the conclusions of the study were related to the assumptions in such a clear cut way that the policy makers could combine the study with their judgment or experience. However even though it is very common for an analyst to make pro forma remarks to the effect that the policy makers will have to use judgment in interpreting the study, usually nothing in either the presentation or concept of the study makes it easy or even possible to do this. It is very much as if the

analyst thought that judgment was like salt or pepper, something to be added at the very end to bring out the flavor when all the other work is finished.

In the illustration, we see a young man dancing with a dummy. He is either desperate or guilty of Mndelism.

We could just as well have shown a young man looking at pin-up pictures, or any seemingly pleasant situation where somebody is playing with or studying an ideal in preference to the real thing. It may or may not be desirable for a very young man to construct his love life around fantasies, but the mature heterosexual male wants a girl! There really is "nothing like a dame."[1]

Lumping together with Modelism the closely related diseases Analysitis and Technocratism we should make the following remarks. Being mainly interested in mathematically clean models, analytical tools, or technical problems is not so much a mistake as an example of a misplaced profession. Technical people with specialized training, knowledge, and capabilities like to use their talents to the utmost. There is nothing wrong with this. Historically many important advances have been made by people whose main interest in a problem was either that it gave them a chance to use skills, equipment or tools that they had or because the problem happened to be personally intriguing. Scientists are motivated as much by capabilities and intellectual curiosity as by being confronted with serious practical problems that demand solution.

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[1] One of our colleagues points out that the analogy is unfair to the Systems Analyst. There are delectable girls all around to tempt our "mature heterosexual adult" away from his dummy, but what can our poor Systems Analyst replace his model with? Another one! Even if he wanted a war he couldn't have it. (Of course, as any psychologist will tell you, the comparison is not so unfair. Some fantasies are nicer than some real girls.)

Unfortunately while the methods and points of view of Systems Analysts are very similar to those of Scientists, the substantive content of the two fields is very different. A well-performed physical experiment has an almost eternal value. This also tends to be true of theories in their region of applicability. Finally, even when their idealizations are so inaccurate as to be almost valueless in predicting experimental data, other scientists may be interested in the work either as a stepping stone to a later better theory or because the problem has become classical. Most systems analyses though are very temporal. If the results are not directly and immediately applicable it is rare that they will have any continuing interest.

For this reason, it is usually sterile to emphasize technical tools in an analysis which is designed to influence policy. In spite of this, many analysts do become enamoured of intellectual and mechanical gadgets, particularly the more modern ones, such as high-speed computers, war gaming, information theory, linear and dynamic programming, differential analyzers, game theory, Monte Carlo, etc. They are easily seduced into emphasizing the use of such tools rather than focusing attention on the real problems. People so oriented are sometimes just salesmen; more often they are serious technicians who may advance the state of the art--in this case they may even turn out first rate component studies. However, they rarely turn out good complete and realistic analyses. This is a criticism only if the analyst is trying to influence policy; if he is trying to advance the state of the art or consciously introducing new tools, then his activities should presumably be judged on a technical basis and it is not necessary for him to introduce substantive considerations.

There is, of course, no objection in principle to esoteric techniques. Both the more technical disciplines and the high speed machines play at least a minor role today and will play a larger and more explicit role in the future. We are only pointing out that they are not central, and usually hardly even peripheral, in Systems Analysis today.

However, the "newer disciplines" are a powerful aid to the understanding and intuition. They also have important applications in some types of Operations Research outside our rather narrow definition of Systems Analysis. Finally we should remember that such essential skills as algebra, calculus, probability theory, statistics, and elementary economics were once considered esoteric.

Therefore, nothing that has been said about the current value of specialized techniques should be taken to imply that research in those fields should not be supported. Indeed many of the objections we have are based on the fact that these new techniques currently have a highly limited capability in applications. As their limitations disappear because of further research and development, they will become correspondingly more important.

In order to make our point clear, it is useful to make one of the same distinctions in the methodology of Systems Analysis that one makes when talking about real systems. This is the distinction between research, development, and procurement. The first would correspond to basic work in techniques or models. When evaluating this kind of work, one does not worry about a pay-off in terms of immediate application but rather one asks if the work is technically and professionally competent and, at least roughly, in the right area. It is, of course, true that one may want to guide the research, but the guidance should be of the loosest and most general sort.

On the whole, the professional should be allowed to do what he is interested in and thinks is important.

Development would correspond to taking techniques or models that are fairly well understood and doing whatever work is necessary to put them into condition for use in applications. Here, of course, the main worry is the tendency to overestimate the performance that can be obtained and to underestimate the time and work that will be necessary. Of course, this kind of over-optimism is important only if there is some sort of deadline or desire to be topical. If the study is being done just for fun, it is not serious.

Lastly, procurement would correspond to work that is being done with the idea of directly trying to influence policy. One should then, as much as possible, stick to on-the-shelf methods. The emphasis should never be on the tools or on making fundamental advances in the state of the art but rather on the important assumptions and the crucial ideas; where "important" and "crucial" mean from the viewpoint of the policy maker.

It is of course well known that the distinction between the three kinds of activities is not sharp. There is a great deal of overlap. It is, however, still very important to make the distinction. There are more remarks on this same subject in the section on Overambition.

Over-preoccupation with analytic details often shows up in another way. The analyst will ask: "What is going to happen?" or "What shall we do after the dust has settled?" While both of these questions are interesting, it is probably a mistake to emphasize them. The most important question is "What can we do about it in advance?" Naively, one might suppose that he would have to find out exactly, or at least pretty accurately, what's going to happen, before he can recommend remedial action, but this does not seem to be

true in practice. Systems Analysis is therefore an unfortunate name; Systems Design[1] would be better.

Even when one is mainly interested in analysis it may be a mistake to spend too much time on relatively well understood details or complicated models. At least in the early stages of a project, if one has to choose between the rough treatment of many models and the detailed and careful treatment of one, the former is usually better. Detail is mainly important in turning up misconceptions and mistakes. That is, one may have to investigate a lot of details in deciding what assumptions to put into the model, but the model itself should be as free of detail as practical.

We should make the caveat that when one has gotten down to the point where he is actually going to advocate the adoption of some preferred system, then it is important to treat as many details as are necessary to make a convincing and reliable story. The point is that the relatively complete treatment of the interaction of details comes late in the analysis if at all, not early. We repeat, in the early stages detail is important only as it affects problem formulation and the continuous re-evaluation, as the study progresses, as to what the problems really are.

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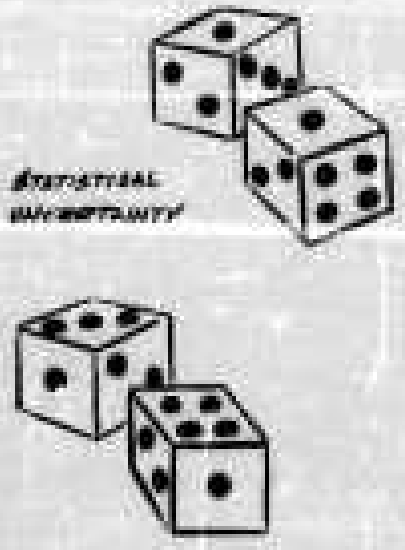
[1] We are indebted to Albert Wohlstetter for pointing out to us the extreme importance of emphasizing design over analysis. While the point may seem obvious, it is surprising what a difference it makes in one's approach to problems. For example, if one is studying the bombing of civilians, then it may be crucial to find out where the civilians are likely to be when the bomb goes off. If, however, one is designing good shelter programs one can merely assume that the civilians are in the shelter. It is part of the design problem to figure out good ways to get them there. This last problem is not only simpler than the first one, but also a more fruitful one to work on. (It should be clear that a well designed system is almost always easy to analyze. If its performance were not clearly satisfactory it could scarcely be a good system. Therefore, if one concentrates on design and is successful, the analysis often takes care of itself.)

In particular the improved treatment of fairly well understood components, while useful, should be deferred until the general picuture is reasonably clear.[1]

For comments on particular techniques, we refer the reader to the previous chapters on Techniques of Operations Research.

[1] We believe that there are at least two exceptions to this rule and both of them concern general self-education projects. The first is the rather specialized and limited study where the analyst might start by "getting his feet wet." The second is the case of a large war game as discussed in Chapter 11. Also, of course, technical studies are almost always concerned with detail.

II. STATISTICAL UNCERTAINTY



The second pitfall is illustrated by two pair of dice, one pair indicating good luck and the other bad luck. This is the kind of uncertainty about which books on probability theory are written. One can write equations and formulas when discussing it, and generally everybody involved will agree with the technical discussions.

In many practical problems, the only way to analyze the effects of Statistical Uncertainty is to do Monte Carlo calculations. While these are often convenient and useful, there seems to be a definite tendency to exaggerate their importance or necessity. In many cases simpler expected-value calculations would be satisfactory. The work that is saved might be better used in other parts of the analysis. In addition, we notice that many Monte Carlo problems are being done with no attention to the principles of good experimental design.[1]

Where Statistical Uncertainty is important, it usually is because it affects Low and High Confidence measures. A High Confidence measure is one on which we can rely--one in which we can have, say, 90% confidence that it will succeed. It is the kind of measure we are always striving for. We

[1] Chapter 9 on Monte Carlo illustrates typical ways in which one can design these calculations to be efficient.

often accept greatly reduced average performance because we prefer effective certainty of a satisfactory result to even a good possibility of a real coup.

A Low Confidence measure, on the contrary, is one which is not likely to succeed, but may if we are lucky. Usually its main purpose is to deny the enemy the possibility of getting a High Confidence measure for himself. If we have at least a 30% chance of success, then by simple arithmetic the enemy will have at most a 70% chance of success. He is automatically denied a High Confidence measure. If we then succeed in making him even a little uncertain, he may be deterred. After all, the stakes are pretty high. Therefore, Low Confidence measures can be very important in deterring the enemy and should not be ignored. They are not, however, substitutes for High Confidence measures if these are available, but are to be considered as a sort of desperate last resort or a bonus. Sometimes they are so cheap that we add them to a High Confidence system in order to be in a position to exploit the occurrence of improbable or uncertain events.

To summarize, one may wish or need to treat Statistical Uncertainty explicitly in order to treat fluctuation phenomena accurately or to look at probabilistic objectives. Unfortunately, however, the explicit introduction of Statistical Uncertainty usually complicates the analysis. Therefore it is always worthwhile to consider doing simpler expected-value studies--possibly deferring the more accurate probabilistic calculation until the qualitative aspects of the problem are fully understood. It may turn out then to be unnecessary to do the more complicated calculations. In any case, if Statistical Uncertainty is treated by Monte Carlo, some attempt should be

made to use good experimental design. A Monte Carlo problem done in a completely straightforward fashion is almost prima-facie evidence of insufficient thought.

III. REAL UNCERTAINTY

To avoid possible confusion, we should start this section by mentioning that usually a measure is classified as having High Confidence or Low Confidence, not because of Statistical Uncertainties as implied in the last section, but rather because of a more fundamental kind of uncertainty which we have called Real Uncertainty. This is the kind of uncertainty to which one might possibly assign subjective probabilities, but for which it is impossible to obtain general agreement on the numerical values of these probabilities. They are more a matter of taste than of calculation or investigation. It is mostly because of the presence of Real Uncertainty that we de-emphasize the lengthy or arduous treatment of Statistical Uncertainty.

Real Uncertainty is the kind of uncertainty that is most likely to cause nightmares. It involves such questions as:

How many bombs will the enemy have? What size?

How many planes? Secret bases? Tankers?

How good is he? Will his skill change?

What surprises does he have?

How good are we??

Will we have warning of the attack? How much? Will we believe it?

Will the Yugoslavs defect? The British? Texas? Brooklyn??

As we can see, Real Uncertainty is quite different from Statistical Uncertainty. To make the contrast specific, let us review a portion of the example.

If we assume that 12 planes are attacking us, that the probability of shooting down any particular enemy plane is .5, and that what happens to each plane is independent of what happens to any other plane, we can then discuss in a learned and objective fashion exactly what the probability is of any particular number of planes getting through. In fact the chart on page 46 gives these probabilities. Therefore even though we are uncertain as to the exact number of planes that will actually penetrate, we know a good deal about the performance of our defense system. However, if any assumptions on the probability of penetration is uncertain then our ability to calculate what mathematicians call binomial probabilities may be almost irrelevant. Typically, in fact, the uncertainty in the probability of survival dwarfs the effects of statistical fluctuation.

If the analyst recognizes this fact then he may be justified in neglecting complicated details that affect only the Statistical Uncertainty. He can then free himself to concentrate on the more serious problems involving Real Uncertainty.

There is no general recipe for handling Real Uncertainty; the Systems Analyst may try several things.

1. He may decrease the area of uncertainty by

   .Doing more by analysis and less by assumption
   .Gathering together as much and as reliable information as possible

·Recommending or arranging that important decisions be deferred until some of the uncertainty disappears (providing that the extra costs incurred because hedging is then necessary are reasonably small compared to the risks involved in making an immediate decision)

2. He tries to design the system in such a way that its performance will not be a sensitive function of the unknown probabilities or parameters. This may involve compromises that result in at least a small loss in performance under certain conditions but it is absolutely essential to try to make the performance of the recommended system insensitive to variations of controversial assumptions. We indicated when discussing Contingency Planning (in Chapter 3) how this might be done.

3. Rather than accept extremely undesirable compromises he considers desperate measures for desperate situations. For example, there are at least two ways to hedge against a possible loss of advanced bases for a strategic air force.

·Design the system so that one doesn't need them. This usually means giving up all the advantages that accrue from having them.
·Plan on some one-way missions (or one-plus) in the unlikely eventuality that the bases are lost. This is a pretty extreme measure and people don't like to consider it in peacetime. However, if a war breaks out some such measures are almost always necessary. World War II is full of examples.

4. He uses "break-even" analyses and "a fortiori arguments." That is he finds reasonable limits and argues, "If this measure is at least this good and this good is satisfactory, this measure is worthwhile," or going the other way, "If this is the best it can be and 'this beat' isn't good enough, . . ."

The illustration shows three ways not to treat Real Uncertainty:

1. One should not be deceived by appearances (as in the old shell game) and assume that Real Uncertainty is statistical. The pea is

practically never under the shell we pick; in fact, it may not be under any shell at all.

2. One should not wear a blindfold and ignore these annoying uncertainties. There are many ways to do this ignoring, two of the common ones being to use official figures blindly, or to accept uncritically technical estimates or ground rules. It is not that these are necessarily incorrect, only that they are not necessarily correct and the analysis should explicitly take account of their uncertainty.

3. It is wrong to prepare for the variations of weather by always wearing trunks and carrying an umbrella. You will be both wet and cold on rainy days and have to drag an umbrella around on sunny days. You will never be suitably dressed for any occasion.[1] One should wear a reasonably compromised and easily modifiable outfit--carrying the special equipment when indicated, but being handicapped as little as possible when the weather is normal.[2]

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[1]Harvey Lynn (the original author of the next chapter "Nine Helpful Hints") suggests that, on the contrary, bathing trunks and an umbrella are perfect for a stroll along a beach where the seagulls are particularly impolite and impartial.

[2]Some of our colleagues have suggested that it takes about as much courage to make this statement (and some of the others) as to make a rousing declaration in favor of motherhood. This may be so, but if one sees a man beating an elderly woman and tries to stop him, he should not be particularly put out if the bully sneers, "Ah, you're one of those guys who are always coming out for mother, home, and country." The point is that even when we do not show explicitly how to treat a problem, we feel that it may still be useful to point out that the problem exists and should be taken account of. Where we can do more, we do.

IV. ENEMY REACTION

It is obvious that our problem would be simplified if the enemy were not trying his hardest to thwart us. Our planning is complex mainly because his reaction and its effect on our design must be considered. In spite of this it is very common to treat the enemy as being supine or inert. Much less common but almost as bad is to treat him as being omnipotent.

The illustration shows the two standard ways of misjudging the enemy. The first is to assume that he is a sort of cretinoid idiot, who can't see, think, or anything. It might be a fair, if dangerous, assumption that the enemy is at least as stupid as we are. However, the chronology is such that, if the enemy has the initiative, he has relatively more time to learn about our mistakes than we have to fix them. Thus, while our defenses are being procured neither of us may see how they can be circumvented. By the time the system is put in operation, we may both have learned a good way to attack it. This helps him but is too late for us. In this respect, we have to be smarter than the enemy and find out about our weaknesses early.

This brings up the whole subject of measures and countermeasures. There is a curious problem involved here. In the early stages of a design you

can't afford to digress too much into the measure, countermeasure, counter-countermeasure, etc., situation.[1] You have to get along with the job of design. At the same time you can't afford to commit yourself to systems that will turn out to be easily countered. A judicious mixture of faith and caution seems to be needed: faith that seeming difficulties will yield to research and development, caution because they may not.

To continue with our discussion of the cretinoid enemy. There is a very common mistake worth special mention. Assume that Intelligence, for example, has told us that the enemy plans to procure a fighter that will go up to 35,000 feet. We, therefore, build our bombers to fly at 40,000 feet where they can look down and laugh at him making passes below. But it takes us two years to develop the bomber and it takes him only five years to develop a fighter. As soon as he learns of our plans, he of course develops a fighter that flies at 40,100 feet (if he is foolish) and blithely shoots us down.

Another form of almost exactly the same mistake appears in many analyses as follows. Suppose it is clear to everybody that a weakness or hole exists or is developing in our defenses. The analyst argues that the enemy would be foolish to plan on attacking us through this hole. After all the enemy is presumably conservative and shall figure that by the time he has the capacity to exploit the hole, we will have plugged it up. So far, so good. But now the analyst relies on the assumed enemy calculation and does not bother to make a strong recommendation that the weakness be fixed. In effect, we are

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[1] The situation can get very confused when there are a lot of people assigned to counters, counter-counters, counter-counter-counters, etc. We are told that in one case two special laboratories were set up--one to get the odd counters and the other to get the even ones. You will recognize that this is a tremendous step forward--organizationally speaking. All that remains are the technical problems.

relying for our defense on the dubious assumption that the enemy will never exploit uncertain events or risky strategies. The trouble is that even though both the enemy and we have figured on paper that the hole is fixed, he may send real planes against our paper.

The reverse of the above is also true. People will argue against a certain measure because it looks like the enemy could easily counter it. They are probably right if there is a satisfactory High Confidence alternative available or if the Low Confidence measure is expensive. However if it is reasonably cheap, then it may be sensible to buy it. One is then at least in a position to exploit the situation if the enemy doesn't use the countermeasure, or what is sometimes even more important, you are in a position to profit if you happen to think of a counter to his countermeasure.

The opposite of the Simple Simon enemy is also shown on the chart, the assumption that the opponent is a giant seven feet tall with four arms, each with two biceps. Each arm can, of course, be used independently and simultaneously.

Clearly it is impossible to design a perfect offense or defense. All that one can do is design a defense which will take a certain level of attack, or an offense that will penetrate a certain level of defense. Therefore, it is crucial in trying to judge how satisfactory a proposed system is to evaluate what these levels are and see whether the enemy can exceed them. But if we don't charge the enemy's resources for the actions he takes, then it is obvious that he will overwhelm us. We must allow the enemy to spend the resources he is entitled to spend, but only those resources.

It probably is a mistake to rely very much on Intelligence estimates when trying to estimate specific future capabilities. We cannot ourselves

predict even roughly the capabilities of our military establishment in 1965 or 1970, even though we presumably know everything about our current resources and plans. Our ability to predict is even more hopeless with respect to the enemy. About the best that can be done is to let him work within the limits set by his gross national product. This means doing a pretty big study all by itself but the alternative generally means relying on some pretty arbitrary assumptions. We may give him some black marks for any specific technological bottlenecks that we are pretty certain he has, but we should be just about certain that these bottlenecks will continue to exist before being willing to rely on them.

In other words, we don't try to estimate his air force size by first estimating his air force capacity and his air force capacity capacity, etc. There isn't much point in trying to predict the number of airplanes he will be making in 1965 by estimating in detail the number of engineers, skilled workers, farmers he can divert from raising beets to welding aluminum, etc. We haven't been able to do this kind of analysis for ourselves where we presumably have relatively reliable information, so it is very improbable that we can do it successfully for him. It is, however, reasonable to take his gross national product, allocate a reasonable percent to his military budget and then divide this military budget among all the things for which he must spend money.[1]

We might do this division in at least two ways:

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[1] This is one problem that is much rougher for the Russian Analyst than the U.S. Economically the U S. could probably support a military budget of over one hundred billion dollars a year without undue strain on the economy. Such budgets would, of course, cause serious political and social stresses. Therefore, the Russian Analyst has to predict the political climate or be very conservative and overestimate our budget. If he does the latter he may make his job impossible.

1. In the accustomed way he has been doing and which seems to fit his military doctrine.
2. In the way which we think is most effective against our plans.

This last method of calculating his military establishment may, for example, overestimate his ability to actually divert agricultural resources into building planes; but if you are talking about a five or ten year period, it often overestimates surprisingly little. It is an occupational hazard of the experts that, because they are completely familiar with the extreme difficulty of shifting resources from one activity to another, they are always underestimating its feasibility. There are, however, literally hundreds of examples where, by use of much work and ingenuity, engineers have triumphed over difficulties which in their calmer moments they would have thought insurmountable. While it is ridiculous to credit the enemy with unlimited capacity, it is probably not very wrong to credit him with an unlimited capacity to shift his gross national product around over a reasonable period of time. He still has to support his civilian economy of course.

As a final example let us consider the problems faced by an analyst who is studying the defense of continental United States. He must always remember that we are not trying to prevent ourselves from being destroyed in some specific manner, but from being destroyed in any way whatsoever. He must, therefore, consider all the possible ways in which an intelligent and determined enemy can attack us. He must not only plan on protecting us from massive raids over the North Pole but also from massive attacks from

the sides and from underneath, and from sneak attacks anywhere. It is true of course that if the enemy attacks the long way around, he is forced to buy a lot of tankers and thus he may not be able to afford as many bombers. But if it pays him to do this, we must (for planning purposes at least) allow him the option. Similarly, we must protect ourselves not only from high altitude and low altitude attacks launched from bases located in the enemy's country but also from planes or missiles launched from submarines or ships at sea. We must worry about countermeasures designed to jam and confuse our radar networks. We must take into account the possibility of spoofing and deception. We must even worry about sabotage. Finally, we must consider defending not only against what our opponent has today, but against the equipment and tactics he will design when he knows what our current defenses are.

Experience has shown that generally planners are very reluctant to credit the enemy with all this freedom. They feel that there is no reason to believe that he is as smart as all that. The point is not that we think the enemy is a combination of Machiavelli, Clausewitz, and Einstein. It is only that we don't want to rely on his being stupid. If he is, fine! If he's not, let's be prepared. The other error, giving the enemy too much credit, is less common but just as serious. If you don't set reasonable limits at least on his physical capabilities, you yourself will be paralyzed and do nothing constructive.

V. OVER-CONCENTRATION

It is usually necessary when studying a component of a larger system to decide what the real problems are so that one can concentrate on them. Unless this is done, the study may be hopelessly complicated. However, there is a real danger that the factoring out of a suitable area will be done carelessly or unskillfully and an overly-narrow viewpoint adopted. One can then end up by working a wrong or irrelevant problem. We have no objection to the driver in the illustration looking at the blonde--she is worth looking at--but not exclusively.

If, for example, one is considering the design of a missile force, it may be wrong to consider the missile system separate from the bomber force. In principle they should be considered as a whole and only after one understands the interrelationship between the two systems should one risk factoring the problems. In actual fact, the Systems Analyst may have neither the time nor capability for doing the complete study and must therefore do some of this risky factoring even though he doesn't really understand the problem. It is very helpful however to do at least a little thinking about the other system and leave some of the questions and conclusions open-ended.

The problems of a modern military establishment are really manifold and

complex. It takes a wide range of measures and instruments to accomplish its objectives satisfactorily. In particular this means that if one is explicitly looking at any single element of a strategic bombing system, such as heavy bombers, light bombers, fighter bombers, ballistic missiles, cruising missiles, etc., one must realize, at least implicitly, that the other elements also exist. Presumably we should even include the Army and Navy.

There is another kind of Over-Concentration which is related to Modelism, and which we might call Assumptionitis.[1] The assumptionist starts by assuming away the difficult parts of the problem. Having made his assumptions, he spends all of his time drawing conclusions from these assumptions rather than in investigating them. It is an understandable trait of the human mind which makes us dislike reopening questions that have been supposedly settled. Very little work would get done if we were completely indecisive. However, when one is planning five to ten to fifteen years in the future, very few questions get settled permanently and one must be alert to the possibility of reopening them.

A particularly insidious form of Assumptionitis is to take an important desired property of the system as a ground rule from some authoritative group, and forget that the ground rule will not come about unless a lot of decisions are made or unless some research and development program is successful. If the ground rule plays an important role, the analyst may want to do two things. First, he should take explicit account of the uncertainty. Secondly, it is often a good idea for the analyst to include in his study the actions needed

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[1] John Turkey points out that the terminology may be misleading. Assumptionitis means, "pain in the assumptions." The trouble is that the Assumptionist doesn't have any.

to make the ground rule materialize. Even if the ground rule seems to be a firm "requirement" it does not mean that people will fight for it when it comes time to budget money. Therefore, the relevance and importance of all future policy decisions should be shown clearly and convincingly if one is to induce people to take original or remedial action. There is a risk antagonizing the audience if one emphasizes what may seem obvious. But if you don't you run an even greater risk. You may find that important actions are not taken, even though everybody concerned seems to agree with the recommendations. After all we are dealing with a large organization in which many decisions are necessarily made by default.

The last remark assumes that one has already won the respect of his audience by previous work and is therefore reasonably respectable. If one hasn't, it may be wiser to limit one's ambitions and not clutter the briefing with what may seem obvious to the audience.

Another place where studies often are confused or misleading because they haven't taken a broad enough point of view is in the treatment of objectives. This usually occurs because most studies consider the single highest priority objective and ignore the many different kinds of objectives our military establishment really has. Let us look at some typical and important Department of Defense objectives in a little detail to see how they have both complementary and contradictory aspects. The authors would argue that our four most important military objectives (in a rough order of priority) are:

1. Deter the enemy from launching an attack on the U.S. or areas vital to it (depends on the certainty and effectiveness of retaliation). We referred to this previously as Type I deterrence.

2. Deter the enemy from provocative action. In particular, deter him from attacking friends or neutrals (depends on being able and willing to meet him on the spot or letting him know that there is a non-zero chance you will take some drastic action even though his misbehavior is not aimed directly at you). This we called Type II deterrence.

3. Be able to win a war in a satisfactory fashion (depends on preserving something worth preserving).

4. Be able to win a war in an unsatisfactory fashion (history awards you the decision--if there is a history).

Any broad context study should evaluate the recommended system in the light of all of these objectives and others, sometimes with emphasis on the by-product values. For example, let us discuss further how one can meet the above objectives.

1. You deter the enemy from attacking you by convincing him that the risks are too great either because you have a capability of absorbing his attack and striking back hard or because you have an ability to get strategic warning and forestall him.

2. Your military ability to deter him from misbehaving in areas which don't automatically start World War III is measured by two things--your on the spot capabilities, as in Korea and Indo-China, and your known or implied willingness and capability to take other actions. For example, in the first World War the Germans knew that the British had a capability for declaring war but did not think they were willing. In the second, Hitler knew they were willing but did not think much of their capability. As a result they failed to deter Germany from provoking a war either time.[1] There has been so much emphasis on Type I deterrence that people sometimes overlook the necessity for having some Type II deterrence in addition to Type I.

3. To win a war satisfactorily today probably depends on either having a stroke of luck, having an effective Civil Defense program, or on its not being much of a war (i.e., the war is limited), so that it is possible to talk about satisfactory outcomes. This ability adds

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[1] Of course as we mentioned previously (note on page 121 in Chapter 4) there are many other things besides the threat of military action which deter a potential agressor from provocative action. However on the other side one's willingness to declare war in the thermonuclear era is decidedly small.
The above is somewhat oversimplified version of history but it serves to illustrate our point so we left it in.

to our Type II deterrence. If the enemy feels that you think that you might be able to win a war satisfactorily, then he is much more likely to credit you with a low reaction threshold. If he is convinced that you are irrevocably committed to not going to war except under the most drastic circumstances, then he presumably feels free to act almost as he pleases. (This is one reason for a Civil Defense program that at least gives one a capability for putting people and essentials into a safe place before taking any action. If one can't do this, the enemy is not likely to worry much about the possibility of receiving any ultimatums or attacks.)

4. To obtain a technical win one must be able to win the all-out slugging match. This ability is probably important only because it reinforces our Type I deterrence by preventing the enemy from winning satisfactorily. One has to be pretty altruistic, idealistic, or bloodthirsty to have a great deal of interest in this fourth objective for its own sake.

A properly designed system can often contribute very effectively to multiple objectives. Sometimes in fact the system will be more valuable when evaluated in terms of an objective other than the one for which it was originally designed. For example, we have seen many systems considered for Type I deterrence which were in effect tied to our ability to obtain strategic warning and were evaluated with this possibility in mind. While such systems can be valuable, in some cases it might be better to consider how the system could contribute to Type II deterrence. In this case you get strategic warning automatically because then you either go first, deliver an ultimatum, or put special forces on an alert.

We should also mention here, even though we will repeat it later, that anything that subjects the enemy to large costs may be worth doing. It effectively reduces his strength by causing him to divert and waste resources.

For example, people sometimes make the statement that, "We shall not strike the first blow." They may take this statement so seriously that they

advocate giving up completely all the elements of our offensive strength that are useful only if we initiate hostilities. They forget that the enemy has a tendency to look at your capabilities and not your intentions. If you have offensive forces he must divert resources from his offense to build up his defenses. Thus the mere existence of even pure offensive forces can help your defense. They may fulfill an important purpose even if they are wiped out on the first day of the war without ever going into action. These forces also contribute appreciably to Type II deterrence by making your opponent more apprehensive and therefore presumably less willing to provoke you. There are of course important political effects which must also be considered when one is studying such offensive forces. In particular, if you are trying to mollify your potential enemy, . may not pay to keep his sleepless--and then again it might. Also, the existe.. uch forces may also keep some of our allies sleepless. This, in spite of the fact that our ability to have Type II deterrence is of more direct value to them than to us.

VI. PHASING

Our picture shows an Air Force officer glancing coyly at the past (Civil War) and the future (Buck Rogers) but not really interested. This is, of course, a fairly serious mistake. One does not buy a military organization from a department store as a unit. A new system must be developed, procured and maintained and is expected to have a lifetime of many years.

In particular, in discussing new weapons systems we should always consider how best to exploit the real estate, equipment, and organizations that we have inherited from the past. These may be avail[illegible] either "for free" or at greatly reduced cost. This does not mean that one should accept everything that's "free." There are a lot of white elephants around.

But if it is cheaper to get a certain kind of performance by adding to or salvaging an old system rather than buying a brand new capability then, other things being equal, we should use the old system.[1] This is true even if historically the old system costs more. Money spent is money

[1] Since othe. things practically never are equal, the choice (considered as a choice) may not be as simple as we have indicated. The only point is that one should take explicit account of the salvage value of the old system in evaluating new designs.

spent. The Systems Analyst is concerned only with future expenditures. Therefore, in comparing two alternatives we should compare only the new costs and not the total costs.

We must not only exploit our inheritance, we must worry about the future also. This does not mean that we should seriously compromise our capability today in order to lay a groundwork for ten years in the uncertain future. It does mean that we should be aware of the future and be willing to make at least small concessions in today's performance if they seem to result in a greatly increased legacy value for our system.

If we are talking about a future system to be introduced say in 1960 then we shouldn't use only 1960 planning factors in our study; more particularly we want to worry whether our system or modifications thereof will still be good in 1961 and 1962 and maybe even 1965 and 1970. The way Research and Development really goes the system probably won't be operational until 1963 anyway. It is all too common to base plans on conditions that are supposed to exist when the new system is first going to be introduced and not on conditions that will exist during its lifetime.

The pace of development today is so rapid that it is important to accept early the full implications of changes in technology and tactics. The "battleship" admirals of the pre-World War II period are now classical. It is less realized that much of the doctrine that came out of World War II may have about the same relevance to a possible World War III as the battle between the Merrimac and the Monitor had to Midway and Coral Sea. One can presume that the American admirals read with interest naval accounts of the Civil War, but one can also assume that they were careful in applying any "lessons"

in these accounts. Current operational exercises may also be unreliable guides to development planning. This is one of the reasons why it is wrong to let operational commands have an overriding influence on development decisions. There are several other reasons.

The operational people have the responsibilities of being prepared to fight today. Anyone who must focus his attention on day-to-day matters is unlikely to be really interested or knowledgeable about the longer range questions. Finally the old problem of not being able to see the forest for the trees also comes in.

It is said that the principles of war don't change. This may be true, but specific applications do. So far as World War III is concerned, we are all--military personnel, civilian scientists, and others--more or less amateurs. The important thing is to recognize that there are large and important areas where nobody has "experience." Not only must we keep abreast of technology, but what is somehow harder, of the implications of technological changes for our operating and strategic concepts. This requires a continual and active re-examination of our beliefs which is the antithesis of the traditional passive attitude.

For example one of the most serious difficulties with which our defensive system must cope is that defense is a reaction to possible enemy threats. We tend not to evaluate these threats early enough. This is partly because of doctrinaire reasons (offense vs. defense) and partly because of lack of imagination. Generally by the time these offensive threats materialize, we have just begun to prepare to meet them. By the time we are prepared,

new ones have materialized. This means that we are always in the process of eliminating weak spots after they develop. It also means that there will always be weak spots as long as the situation is changing. It is the major job of the Systems Analyst to reduce these time lags. If he is good, he will often seem to be talking about solutions to problems which are still in the never-never land (i.e. 5-10 years in the future; 15-20 years really is a never-never land).

VII. OVER-AMBITION

If one is at all conscientious it is easy to fall into the pitfall of trying to do too big a job. Many of the points raised in discussing Real Uncertainty, Enemy Reaction, Over Concentration and Time Phasing tend to lead the analyst in the direction of increasing the amount of work he has to do. It is, however, immediately apparent that one of the main tricks in turning out a good analysis is to spend a lot of time inventing questions which can be usefully answered within the capabilities and time available.

It is essential for the analyst to realize that it is important for him to stick to problems on which he really can give sound and extremely defensible advice. Many analysts take the position that if an executive is faced with a problem then it is their duty to give the best advice they can--particularly if the executive indicates he would like some. These analysts sometimes argue that they are at least as smart as the executive, therefore why shouldn't they be heard. This may be so but the authors just can't get excited over the blind leading the blind. They firmly believe that it is almost unprofessional behavior for an analyst to point when he can't see irrespective of the visual acuity of the rest of the community.

It should be clear, therefore, that many of the previous remarks may not hold when one is doing a limited study. Particularly if one is inexperienced or has rather limited resources available, the study should be severely

limited. If it can't be limited, in a sensible way, it probably shouldn't be done. It is a serious error to assume that everybody is a global thinker and has the talent, taste, and resources to treat all of the larger questions.

For example, the decision as to whether one should use a flat-head or Philip-head screw should fundamentally rest on the military worth of the two screws. However, it is futile to ask the engineer who is making this choice to consider all the possible wars in which his equipment might be used. It is necessary instead to set up reasonable criteria which are half-way points to what the real criteria are. These criteria must be sufficiently explicit and specialized that people making limited studies can understand and use them. They must be sufficiently general that the sub-optimizations will be relevant to the over-all general optimization. One of the main outputs of a broad context Systems Analysis should be criteria for use in smaller studies.

The situation faced by the Systems Analyst is quite different from that faced by somebody dealing with the market place. Consider, for example, a manufacturer who is trying to decide whether or not he should build a factory to manufacture bicycles. In deciding this, he should in principle take detailed account of all the factors that affect the bicycle manufacturing business. These factors include the business cycle, the interest rate, power and material sources, tariff policies, population trends among workers and customers, competition from automobiles and tricycles, the effects of television, what his competitors are likely to do, etc. Instead of having to go through this huge detailed analysis, however, all that he really has to do is to guess the market price of his output and what his costs will be. While all of the factors given above go into determining these costs and prices, it is also true that if he directs attention only to the price levels and not on the detailed mechanism that determines them, he is often able to

do a pretty good job. He doesn't have to solve all, or even very many, of the world's problems. In particular he can use last year's selling prices and costs as a datum point and correct it for any effects that he thinks are important.[1]

There is, however, no going market place in which a Systems Analyst can sell his products for dollars of military worth. He must, therefore, set up himself or have set up for him criteria by which he can judge designs. However, the setting up of criteria can be extremely difficult and should not be done casually. In particular if the success of the study depends on being able to analyze large issues, then mature and competent people should be available to do this analyzing.

The environment or context question is as difficult as the criteria problem and is to be avoided by the novice. There are many people who, given a definite objective and a definite context or environment, can turn out a very creditable study. Relatively few are reasonable at setting environments. We have indicated previously that it is easy for big studies to get bogged down in irrelevant or unimportant details. It is even easier for little studies to go astray because the analyst attempted to answer questions that were beyond his (or anybody's) capabilities.

There is a specialized kind of over-ambition that is worth discussing.

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[1] There is another example which is interesting. Consider the problem of estimating the U.S. gross national product 5 years from now. Barring depressions one can get a very good value (within a few %) for this number by assuming it increases about 2.5% a year. But what if you didn't have this year's gross national product as a datum point? Well, economists who first tried to estimate this number from detailed studies were generally off by a factor of about two. If an analysis depends on estimating such a number from first principles then the analyst had better take account of the uncertainties in the calculational method. This is often done by rephrasing the question so that he doesn't have to try to do this difficult calculation.

This is the overly big model that attempts to treat almost every aspect of a problem simultaneously. What often happens is that the analyst finds himself being criticised at every turn because he has left out facets. He is vulnerable to these criticisms because he hasn't any real goals in view so he can't say what's important or relevant. About the only way he can answer objections is to make the model bigger and more complicated. He doesn't stop the criticism but he does find that his model has gotten bigger than either his intellectual or calculational resources.

This is particularly likely to happen if it is a computational model. Often the size of the model was not determined by a study of what is really relevant but by the capacity of the computing machine. People are almost invariably optimistic about this sort of estimate, so it usually turns out that the problem has been underdesigned.

For whatever reason some fairly serious truncations must then often be made quite late in the game. Insofar as the study of universal models advances the state of the art, particularly in the economics or computing fields, there is probably no objection to it. In fact if competent people are doing the study one probably wants to support the activity. But it is not as yet Systems Analysis.

The analyst should beware of another aspect of big computing projects. As we said, the time involved in setting up and programming large problems is almost invariably underestimated. Instead of taking a few months it may take a year or even longer. By this time people may have lost interest. Ideas change fairly rapidly in this field, and questions that people thought were important get settled or are shown to be unimportant. If the over-ambitious researcher has not learned his lesson, he may start again with another big model which will take a year or two to program and once more be far behind the times

when he finishes.

It should be pointed out that one should expect big computing projects to be a precarious method of research in new fields. Ordinarily research on a new problem is a slow process of growth and interaction. There is a gradual increase in the problem area achieved by doing calculations, learning from these calculations, designing new ones, learning from them, and so on. It is usually not worthwhile to use the high-speed machine to take a small step. If the analyst takes a big step he may have to short circuit the learning and doing process. but he may not know enough to do this. It is difficult to decide in the <u>early stages</u> what we are interested in and what we want to do.

It is a trite and common statement that the use of analytical gimmicks is secondary to understanding the problem. It is, however, not generally realized just how extraordinarily secondary they are. If all of the large computing machines were destroyed, a few, but only a few, good Systems Analysts would find themselves handicapped--except possibly when preparing for a presentation. As far as we can see, the main role of the high-speed machine in Systems Analysis today is not as an aid to research or design, but more as a means of verifying and preparing for presentation ideas that were fairly clearly understood as a result of rather small or rough calculations.[1]

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[1]The high-speed computers do play a central role in many physics and engineering subjects--particularly large parametric studies and also in data processing. Such studies may indeed be an important component of a Systems Analysis and to that extent we should apologize for some of our remarks. What we object to is focusing attention on the mechanics of the computer or on the technical ideas rather than on the <u>important assumptions</u> of the study. To repeat a quote made earlier, in many studies, "The workmanship is better than the materials."

When one is preparing a briefing of fairly new ideas, it is very convenient, especially when dealing with relatively unfriendly audiences, to have available the results of a large number of computations. They not only make the formal presentation clearer but it is very persuasive to answer objections by citing specific, detailed, and relevant numerical calculations.[1] This is still true even when the briefer is convinced that a qualitative argument should be sufficient or even better.

Finally, the large-model man always runs into the danger of spending most of his time psychoanalyzing a computing machine rather than studying the real world. He learns a lot about coding and very little about systems.

In spite of the above remarks, it is important to realize that there are many benefits that come out of big projects, computing or otherwise. It is only by tackling hard problems that the state of the art is improved and the limits set. In addition there are almost invariably important by-products. It is not fair to think of these by-products as accidental because practically any large technically reasonable project produces some. However it is worth pointing out that the pioneer rarely reaps much personal benefit from his labors. Pioneering has to be its own reward.

The subject of pioneering deserves a paragraph by itself. It is well known that theoretical physicists and mathematicians tend to do their best work before they are thirty. Economists and sociologists on the contrary rarely hit their stride before the age of forty and at this age their best years are often still to come. Probably the main reason for the difference

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[1] There is an unfortunate bonus effect. Even "sensible" people sometimes are reluctant to argue with a high-speed computer. This attitude seems to be disappearing. War gaming seems to be the current (1956) oracle.

is that progress in the first field is mainly a product of boldness, imagination, and originality. While these qualities are also important in the second field, they need a good admixture of judgment and experience. Systems Analysis as a profession needs both types, but Systems Analysis designed for consumption should lean heavily on the sobersides.

In any case the success or failure of a current analysis should not depend on being able to make big or fundamental improvements in the art of Systems Analysis. As much as possible the two activities should be kept separate. If they aren't, there is a real risk of ending up with neither good art nor good application. This does not, of course, mean that the personnel have to be kept separate--only the projects.

VIII. FANATICISM

Fanatscism is a peculiarly sssy and insidious sin. What we are worried about here is not the man with wild hair and rolling eyes and incoherent speech, who has gone wild about some gadget or technique and expects to solve all problems with it. It is not that this kind of guy doesn't exist--he does-- but he is easy to recognize. His mannerisms and intensity are themselves sufficient to remove him from serious consideration.

We are thinking here of the completely different kind of problem illustrated by the "zoot suiter." The curious thing about the "zoot suiter" is that <u>in his circles</u> he is considered well dressed; his friends like his clothes. In fact, what we are stressing is not the fanatic individual but the fanatic organization.

Almost all organizations are subject to fashion; some are even monolithic. An idea gets popular and everybody hops on the bandwagon. Sometimes, it is only a question of having vested interests or being obviously partisan. Mostly though, it's just the way people (including scientists) are. Very few people can hold tentative opinions about questions they are interested in, particularly if their colleagues have made up their minds. Even the most independent members may be swept off their feet by the

intellectual tide. It may be a little worse in classified work but the other fields are not immune.

Several things can be done to allaviate the situation:

1. Get a competent and honest staff.
2. Make the effective discussion groups for the important ideas fairly large.
3. Encourage independence of thought among individuals as much as possible. In particular be tolerant of lone wolves and mavericks.
4. Provide for frequent and effective outside criticism and refereeing.

Still in spite of everything that is done, there will be a party line. This is probably the most important single reason for the tremendous miscalculations that are made in foreseeing and preparing for technological advances or changes in the strategic situation.

One of the main advantages in having at least some Systems Analysis done by independent civilian organizations is that their non-military nature--and more importantly, their freedom from staff responsibility--make them a little more capable of withstanding pressures for intellectual conformity. A military organization by its very nature is not a debating society and would soon collapse if it were run like one. But about the only way known to avoid intellectual ossification is to allow the greatest possible freedom of debate and discussion and to encourage a diversity of views.

A good organization devoted to Systems Analysis may, at any particular time, have a fairly large percent of its staff devoted to seemingly crackpot projects. The only difficulty will be that different individuals will put different projects into this category. Moreover, even demonstrably

impractical projects can be justified because they may advance the state of the art. In any case they give people a chance to unburden themselves and to discover for themselves what is reasonable and what is not.

In other words, tolerance is absolutely essential. In previous remarks we may have seemed intolerant of some points of view, but after all, we are only exhorting, not compelling. Our remarks are addressed to the customers and the practitioners, not to the administrators. Many of the things which we have been criticising are widely practiced and somewhat oversold, so we feel that it may be valuable to give the other point of view. However, we are nowhere near as doctrinaire or sure of our position as we sound.

IX. HERMITISM

The problems of communication and persuasion are often ignored though they are central to getting recommendations translated into policy. It is really a question of the proper type of interactior between the analyst and the other parts of the analyst's organization and also between the analyst and the world of policy. Any good analysis does four things simultaneously:

1. It provides good criteria and good environment for sub-studies.
2. It provides information for studies with a larger context than its own.
3. The assumptions have been properly handled (see pages 141 to 148[1]) and it is complete enough so that the policy makers directly or indirectly concerned can either adopt the recommendations or understand exactly why they disagree with the Systems Analyst. As we explained it does not necessarily follow that just because people have different assumptions or objectives they will necessarily disagree with the conclusions.
4. It is presented in such a persuasive and educational way that it actually has the impact intended. This last requirement often means the Systems Analyst must not only personally spend a good

[1] RM-1829-1, Techniques of Systems Analysis, by H. Kahn and I. Mann.

deal of time "selling" his study, but that he has the capability of doing this "selling" job. It should be emphasized that most of what is involved in being able to do a good selling job is not learned in a public speaking class or charm school. It is less a question of personality than of being oriented as to where the study fits in and what are the real objections people have.

It is relatively easy to arrange for interaction with one's own organization. It is much harder to arrange for the right kinds of contacts with both junior and senior people in the military establishment. In some organizations there is a tendency to work only with high level people. This is a bad policy. Normally most of the decisions are necessarily made at the staff level.

One of the trickier questions involves under what circumstances one is justified in jumping over the staff members to their superiors. (We are assuming here that the Systems Analyst's organization is outside the chain of command.) We feel that the Systems Analyst should always be willing to do this if the issue is at all important. While his relation to the man he normally works with should be close, he should not feel, or allow the other to assume, that the analyst is committed to not arguing at higher levels. It is of course absolutely essential that the analyst understands and makes clear where and why there is a difference in opinion. One is, of course, almost never justified in jumping any channels clandestinely. However, the only really unforgivable sin is going to the public press or Congress.[1]

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[1] Bernard Brodie has pointed out that public journals and books are sometimes legitimate and even advisable channels of communication. A book which has been favorably reviewed and aroused some comment is almost certain to be read in high quarters. A report may just gather dust. Also a well-received book attains a certain amount of prestige.

Now a minor point. In many discussions of Operations Research, a great deal of stress is laid on the difficulties of communication between the Operations Analyst and his customer. We have observed, however, that some, if not most, of this difficulty is created by the Operations Analyst. He often dresses up his results and attempts, either consciously or unconsciously, to hide fairly elementary notions in extreme mathematical or technical language. Though it is probably not possible to condense the most esoteric results of modern mathematics and physics into the language of the newspapers, this is just not true of any applied operations analyses that we have seen. We concede that it may be necessary for the customer to understand a little probability theory--anyone who understands dice qualifies--and to have about the same knowledge of elementary classical economics that the average grocery store proprietor has. (This is much more than many intellectuals would guess.)

In general, the relationship of the Systems Analyst with the policy maker and his assistants should be one of mutual trust and respect. The relationship should be close, confidential, and continuing.

X. BUTCH

The ever-present possibility of a Butch is rarely discussed but is very important. There are two types--the first and obvious one is the classical mistake in arithmetic. This possibility gets worse with the advent of the high-speed computer (only now it takes the form of a coding error). This kind of mistake can usually be avoided by careful checking and equally careful qualitative evaluation of the results to see how reasonable they are.

The second and more insidious kind of Butch is the completely mistaken technical notion or fact. The Systems Analyst, who is doing a broad context study, may have to work with a large number of experts drawn from many fields. It is crucial in dealing with these experts not to accept their statements uncritically, no matter how scholarly or distinguished they are.

Anyone who has spent any time at all in this field has had the following disturbing experience. He consults his own people and gets a very flat statement from them on what the technical situation is or can be. He then goes to another organization that is also technically competent. There he finds out that the situation is at best controversial, or even that his own people are completely wrong. This should not be treated as a disaster causing one to lose all faith in the competence of the people (though it is clearly not confirmatory evidence of their competence), but as a fairly normal occurrence which can be expected to happen occasionally.

The analyst must generally adopt a two-fold program to keep from being led astray easily. He should make himself at least a lay expert in all the important fields he is concerned with. Only then can he communicate effectively with the professional people on whom he must rely. Secondly, he should consult as many experts and sources as possible. In fact, if he knows how to do it tactfully, he should play experts against each other. He can, for example, relay the arguments himself or eavesdrop on the experts' discussions. A slightly cute, but often very effective, technique is to ask the advocate of a particular point of view how his enemies would attack it. Most competent people are quite clear (when thus pressed) on the weaknesses of their position.

It is often and truly said that good systems analyses are done by teams. This is true in the sense that ordinarily a number of technical people are involved in a large analysis, many of them in an essential way. It is, however, also true that final responsibility should be shouldered by only one, or in some special cases, a very few people. This responsibility includes not only the basic ideas and methodology but also the technical information. The project leader cannot excuse serious mistakes by saying that he relied on information supplied by so-and-so and that the said so-and-so had been given a degree by a reliable university. As far as possible he should personally check both the facts and the man. A mistake or bad piece of judgment by an "expert" can be worse than one by a "novice," because it tends to become doctrine. The need for extreme care is, of course, less acute for pilot or public relations type studies that are unlikely to be the basis for serious policy recommendations.

Once however, the project leader has decided to adopt some point of view he should hold it with a little firmness. He should, of course, keep an open mind in the sense of listening carefully to the opponent's arguments but he should be willing to defend his position vigorously, even when the argument is over technical points on which the opposition is supposedly more expert or at least more distinguished than hi- -m pe-.la. It is surprising how often even distinguished technical peop- -ll raise specious, inconsistent, or even dishonest arguments when they are try -; to argue against some unpalatable recommendations. The Systems Analyst can explore the area of disagreement only by pushing his point of view vigorously and he should be willing to do so.

A last point. While the Systems Analyst must have high standards for the quality of the technical work that goes into the study, the standards should not be so high that they are self defeating. If he insists on checking every fact with every possible person who could have any opinion on the subject, then he would never finish the study. He must do enough cross checking to convince himself that in all probability, he has the corr-ct facts and then take his chances. This means that once in a while h- w-l be misled and that further, if he adopts our previous admonition of defending his opinions vigorously he will look foolish, but one cannot do effective work in this field unless one is willing to take the risk of occasionally looking foolish.

To summarize, while it is true that analyses often are and must be carried out by fairly large teams and that it is important for the sake of both morale and fairness to emphasize the team character, this should never be done to the extent of diffusing responsibility. Systems Analysis is still an art and good art is generally produced by individuals, not committees.

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We close by misquoting W. S. Gilbert on the subject of Project Leaders.[1]

If you want a recipe for that popular mystery,
Known to the world as a Project Leader,
Take all the remarkable people in history,
Rattle them off to a popular meter.
The pluck of Lord Nelson on board of the Victory--
Genius of Bismarck devising a plan--
The humour of Fielding (which sounds contradictory)--
Coolness of Paget about to trepan--
The science of Sullivan, the eminent musico--
Wit of Macaulay, who wrote of Queen Anne--
The pathos of Paddy, as rendered by Boucicault--
Style of the Bishop of Sodor and Man--
The dash of a D'Orsay, divested of quackery--
Narrative powers of Dickens and Thackeray--
Victor Emmanuel--peak-haunting Peveril--
Thomas Aquinas, and Doctor Sacheverell--
Tupper and Tennyson--Daniel Defoe--
Anthony Trollope and Mr. Guizot.
Take of these elements all that is fusible,
Melt them all down in a pipkin or crucible,
Set them to simmer and take off the scum,
And a Project Leader is the residuum!

[1]The Nine Helpful Hints and Miscellaneous Comments chapters have some additional remarks on the Systems Analyst as project leader.

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